What impact, if any, has feminism had on science?[1]

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The women's movement of the 1970's and '80's, or, as it is often referred to, second wave feminism[2], was first and foremost a political movement. It aimed at changing the conditions of women, recognizing full well that to do so meant changing the world. Out of the overtly political project soon emerged an intellectual – and even academic – project: feminist theory. Feminist theory was generally understood, at least by its early contributors, as itself a form of politics – i.e. as "politics by other means." It aimed to facilitate change in the world of every-day life by analysing – and exposing – the role that ideologies of gender play (and have played) in the abstract schema underlying our modes of organization. This meant re-examining our basic assumptions in all the traditional fields of scholarship – history, literature, political science, anthropology, sociology, etc. Being a scientist, I chose to extend the kinds of analysis feminists were employing in the humanities and social sciences to the natural sciences. In particular, I sought to understand the genesis of the sexual and emotional division of labour, so conspicuously prevalent in my own generation, that labelled mind, reason, and objectivity as 'male', and heart (and body), feeling, and subjectivity as 'female', and hence, that underlay the historic exclusion of women from the scientific endeavour. My hope was that to identify such traces of masculinist ideology in the natural sciences would lead to their purging, for surely, here of all places, they ought not be tolerated. It was a heady time, and like so many of my colleagues in feminist theory, my aim was ambitious to the point of grandiosity: perhaps less ambitious than seeking to change the world, I sought merely to change science. Let me explain: my aim was not to make science either more subjective or more 'feminine', but rather, to make it more truly objective, and, necessarily, 'gender-free.' I sought, in a word, a better science. A better science, I argued, would inevitably be a more inclusive science, more accessible to women. In rapid order, this project (which I labelled "gender and science") was joined by many others – some with similar and others with different aims. But we all shared the bottom line commitment to making this undeniably human achievement more inclusive and more humane. Now, a quarter of a century later, it seems appropriate to ask: What in fact did we accomplish? Did we change the conditions of women? Did we change the world? Did we change science?

In many ways, the women's movement of the 1970's and 80's obviously did change the world. Perhaps not in the radical ways some of us had envisioned, but it certainly changed perceptions of women (and of gender) throughout much of the western world. Indeed, it changed more than perceptions, it changed the condition of many women in this part of the world.

More to the point here, contemporary feminism changed the position of women in science. While we still cannot claim total equity, in the US at least, we have witnessed an astonishing transformation over the last three decades. Where in 1970, only about 8% of all doctorates awarded in the natural sciences went to women, today, that number is about 35%[3]. Even more striking is the subsequent success of these women, especially of those who have obtained their degrees in the last 10 years. For example: in 1970, women who were full professors in any of the scientific disciplines were extremely hard to find (most departments had none). Today, by contrast, among the full professors in the natural sciences who have received their doctorates in the last ten years, women account for an astonishing 46%[4].

[1]Based on talks given in Valencia, June 2003; Bangalore, 19 February 2004; and drawing extensively on two previously published essay (Keller 1996, 2001). This article is based solely on developments in North America and Western Europe.
[2]"Second wave" to distinguish it from the earlier women's movement in the US and the UK of the previous century.
[3]see http://www.awis.org/resource/satistics/fig-Doctorates_by_women_field_1970-99.gif for a plot of these numbers from 1970–1999.
[4]However, only 16.8% of those who received their doctorates more than 10 years ago are women. These figures are taken from http://www.awis.org/resource/statistics.html.

How such a dramatic change came about is not mysterious: in large part, it resulted from direct political pressure exerted by women's groups – especially perhaps, by organizations of women scientists in the professional societies. Of course, such strategies were effective because they met a definite need, but I think it is fair to say that this change, at least, was the result of political action.

My question here is: can we claim more? Did we, as I (for one) had originally hoped we would, change science? I want to make the provocative claim that there are some ways in which we did change science, even if, once again, not quite in the sweeping ways some of us had envisioned. To support this claim I will list a number of shifts – all of them in biology, and all of them in obvious sympathy with feminist goals, that took place roughly contemporaneous both with the influx of women in science and the emergence of a feminist critique of science.

These shifts – all of them involving phenomena referred to as "maternal effects" – amount to what I think is the "strongest case" that can be made, and I want to ask how they came about. What, if anything, did they have to do with political action, with the influx of women scientists, or with the writings feminist scholars?

1. Maternal effects in fertilization

Let us start with the simplest example, and thanks in good part to the work of Martin (1991) and of Gilbert and his students (Gender and Biology Study Group 1989), the example that is probably best known, namely, that of fertilization: Until fairly recently, the sperm cell has consistently been depicted as "active," "forceful" and "self-propelled", enabling it to "burrow through the egg coat" and "penetrate" the egg, to which it "delivers its genes", and "activate(s) the developmental program". By contrast, the egg cell is passively "transported" or "swept" along the fallopian tube until it is "assaulted", "penetrated", and fertilized by the sperm (Martin 1991, 489–490). The noteworthy point here is not that this is a sexist portrayal (of course, it is), but rather, that the technical details elaborating this picture have been, at least until the last few years, astonishingly consistent with it: the experimental work provided chemical and mechanical accounts for the motility of the sperm; for their adhesion to the cell membrane, and for their ability to effect membrane fusion. The activity of the egg, assumed *a priori* to be non-existent, required no mechanism, and no such mechanism was found.

Only recently has this picture shifted, and with that shift, so too has shifted our technical understanding of the molecular dynamics of fertilization. In an early and self-conscious marking of this shift, two researchers in the field, Gerald and Helen Schatten wrote in 1983:

> "The classic account, current for centuries, has emphasized the sperm's performance and relegated to the egg the supporting role of Sleeping Beauty. . . . The egg is central to this drama, to be sure, but it is as passive a character as the Grimm brothers' princess. Now, it is becoming clear that the egg is not merely a large yolk-filled sphere into which the sperm burrows to endow new life. Rather, recent research suggest the almost heretical view that sperm and egg are mutually active partners" (Schatten and Schatten 1983, 29).

Indeed, the most current research on the subject routinely emphasizes the activity of the egg cell in producing the proteins or molecules necessary for adhesion and penetration. In a recent issue of *Nature*, e.g. we can read:

> "At one time, eggs were regarded like the cargo in the hold of a vessel. . . . We now recognize that each egg actively influences the development of its own follicle – it despatches commands affecting the growth and differentiation of the granulosa cells around it, while receiving information and nutrition from them. . . . This could throw fresh light on unexplained forms of infertility and indicate new strategies for contraception" (Gosden 1996).

Even the widely used textbook, *The Molecular Biology of the Cell*, seems to have embraced at least nominal equity on the matter: here, "fertilization" is defined as the process by which egg and sperm "find each other and fuse" (Alberts *et al* 1990, 868).

These egalitarian references are not rhetoric – they are based on an account that is now strongly supported by a rich panoply of mechanisms that researchers have identified in recent years – one might say, that researchers have found because they went looking for them.

2. Maternal effect mutations and developmental biology

This story requires a brief historical introduction: One hundred years ago, biologists defined inheritance as subsuming concerns about transmission along with those about development. The central question of what was then called Embryology – how does an egg cell develop into an organism – was also the central problem of biology. But with the rise of the American school of (Morganian) genetics in the 1920's, what had earlier been a single subject became split into two rival fields, genetics and embryology. Throughout the thirties, the two disciplines ran neck-in-neck, but by the advent of World War II, embryology began a decline from which it did not recover. Only in the last 25 years has that subject, and its question, returned to center stage.

Relations between the two disciplines in the pre-war period are neatly captured by a drawing by the Swiss embryologist, Oscar Schotté, depicting two views of the cell: As seen by the geneticist, the cell is almost all nucleus, but as seen by the embryologist, the nucleus is barely visible (from Sander 1986). In this drawing, nucleus and cytoplasm are employed as tropes for the two disciplines – each lends to their object of study a size in direct proportion to their perceived self-importance.

But to speak of the rivalry between two disciplines, troped here by two separate domains of the cell, suggests the possibility of (and need for) co-existence. Geneticists, however, had a program for colonizing the cytoplasm, and with it, the discipline of embryology. That program is captured in a metaphoric field that I have elsewhere described as the "Discourse of Gene Action" (Keller 1995)[5].

By the Discourse of Gene Action, I mean a way of talking about the role of genes in development, introduced in the 20's and 30's by the first generation of geneticists, that attributes to the gene a kind of omni-potency – not only causal primacy, but autonomy, and perhaps especially, agency. Development is controlled by the action of genes. Everything else in the cell is mere surplus. As H J Muller put it in 1926:

> ". . . the great bulk . . . of the protoplasm was, after all, only a by-product of the action of the gene material; its "function" (its survival-value) lies only in its fostering the genes, and the primary secrets common to all life lie further back, in the gene material itself" (Muller 1929).

The discourse of gene action actually evokes a Janus-faced picture of the gene in relation to the rest of the organism – part physicist's atom, and part Platonic soul, at once fundamental building block and animating force. This way of talking not only enabled geneticists to get on with their work without worrying about what they did not know; it framed their questions and guided their choices, both of experiments worth doing and of organism worth studying.

Nowhere is this more striking than in their reframing of the problems of embryology. Alfred H Sturtevant, e.g. was explicit: In 1932, he wrote:

> "One of the central problems of biology is that of differentiation – how does an egg develop into a complex many-celled organism? That is, of course, the traditional major problem of embryology; but it also appears in genetics in the form of the question, "How do genes produce their effects?" (p. 304).

Between "the direct activity of a gene and the end product", he went on to argue, "is a chain of reaction". The task of the geneticist is to analyse these "chains of reaction into their individual links."

This rephrasing of the embryologist's question guided research in developmental genetics for the next 40 years. It encouraged the view that research on "gene action" was primary, that developmental "chains of reaction" could as well if not better be studied in single-celled organisms than in higher organisms, and that cytoplasmic effects were at best of secondary interest – in Morgan's term, "indifferent." The phenomenal success of this research program, first in classical, and later, in molecular genetics, goes without saying. But it also had its costs – not simply in the long eclipse of the discipline of embryology and its original problem, but also of a genre of experiments and even of organisms (the *Drosophila* embryo, e.g.). Over the last 25–30 years, this problem has returned to center stage. And with its return, has come a change in discourse. As we have learned more about how genes actually work in complex organisms, the perceived locus of control has shifted from genes themselves to the complex biochemical dynamics (protein-protein and protein-nucleic acid interactions) of cells in constant communication with each other.

New metaphors abound. Nijhout has even suggested that it would be better to think of genes "as suppliers of the material needs of development", as "passive sources of materials upon which a cell can draw" (1990, 441). Nijhout's proposal is echoed in more recent references to "DNA as Data" (see, e.g. Thomas 2004). There is no question that a new way of talking is in the air, in keeping with the emergence of a new biology: Molecular biologists seem to have "discovered the organism".

How did this happen? This is a large question, and there is little doubt that the introduction of new technologies for cloning and manipulating genes has been immensely significant. But they do not tell the whole story. Consider, e.g. the work on maternal effect genes and cytoplasmic rescue in *Drosophila* begun in the early 70's, and later carried to such remarkable fruition by Christiane Nüsslein-Volhard and her colleagues. This work, establishing the critical role played by the cytoplasmic structure of the egg prior to fertilization, is widely regarded as pivotal in the recent renaissance of Developmental Biology. But it did not depend on new techniques. Indeed, Ashburner writes that "it could have been done 40 years ago, had anyone had the idea. . . . All (it) required was some standard genetics, a mutagen, and a dissecting microscope, all available in the 30's." (1993, 1501). So why had it not been done earlier? Ashburner says no one had the

[5]The notion of "gene action" is by now so much a part of our language that it must be considered a dead metaphor, and like many other conspicuously dead metaphors – e.g., "the first three minutes", "genetic program" – accumulating in power as it declines in vitality (or visibility).

idea, but this is not quite right. Rather, I suggest, it was the motivation that had been missing. These experiments are immensely difficult and time-consuming; one needed the confidence that they were worth the effort. Or put another way, there was no field in which the “idea” could have taken root. Earlier, the discourse of gene action had established a spatial map that had lent the cytoplasm effective invisibility, and a temporal map that defined the moment of fertilization as origin, with no meaningful time before fertilization. In this schema, there was neither time nor place in which to conceive of the egg’s cytoplasm exerting its effects. Indeed, the preferred term for “maternal effects” was “delayed effects.” As long as one believed that the genetic message of the zygote ‘produces’ the organism, that the cytoplasm is merely a passive substrate, why would one go to all that trouble? By the 1970’s, however, the discourse of gene action had already begun to lose its hold. A number of different kinds of changes, above and beyond the obvious technical progress of Molecular Biology, contributed to its decline; here I will mention only three.

I have already referred to Oscar Schotté’s invocation of the nucleus and cytoplasm as tropes for the disciplines of genetics and embryology. In his sketch, each disciplines lends to its object of study a size reflecting not only its self-importance, but also its own self-attributes of agency, autonomy, and power. In addition, however, the nucleus and cytoplasm also came to stand as tropes for national importance, agency, and power, with the former, as the domain in which American genetics had come to stake its unique strengths, associated with American interests (and prowess), and the latter, with European, and especially of German, interests and prowess. German biologists were often explicit about what they saw as the attempt by American geneticists to appropriate the entire field. In 1927, e.g. Haecker described the field between genetics and development as the “no-man’s land” of somatogenesis – “a border field which by us has been tilled for quite some time. . . . The Americans have taken no notice of this” (1927). This tension persisted throughout the interwar years, and was resolved only with the resounding defeat of Germany (and the virtual destruction of German biology) in World War II.

But the most conspicuous metaphoric reference of nucleus and cytoplasm is surely to be found in sexual reproduction. By tradition as well as by biological experience, at least until World War II, nucleus and cytoplasm are also tropes for male and female. Until the emergence of bacterial genetics in the mid 1940’s, all research in genetics and embryology, both in Europe and the US, focused on organisms that pass through embryonic stages of development, and for these organisms, a persistent asymmetry is evident in male and female contributions to fertilization: the female gamete, the egg, is vastly larger than the male gamete, the sperm. The difference is the cytoplasm, deriving from the maternal parent (a no-man’s land indeed); by contrast, the sperm cell is almost pure nucleus. It is thus hardly surprising to find that, in the conventional discourse about nucleus and cytoplasm, cytoplasm is routinely taken to be synonymous with egg. Furthermore – by an all too familiar twist of logic – the nucleus was often taken as a stand-in for sperm. Boveri, e.g. argued for the need to recognize at least some function for the cytoplasm on the grounds of “the absurdity of the idea that it would be possible to bring a sperm to develop by means of an artificial culture medium” (published posthumously in 1918 (p. 466), and translated in Baltzer 1967, 83–84). Many of the debates about the relative importance of nucleus and cytoplasm in inheritance thus inevitably reflect older debates about the relative importance (or activity) of maternal and paternal contributions to reproduction, where activity and motive force were routinely attributed to the male contribution, while relegating the female contribution to the role of passive, facilitating environment. The egg is the body, and the nucleus, the activating soul.

3. Evolutionary biology and ecology

The impact of changing perceptions of gender on the biological sciences has grown steadily over the past two decades, and has come to extend well beyond studies of fertilization *per se*, and even beyond problems of early development. It has come to effect a host of research endeavours in ecology and Evolutionary Biology – all enriched by a new appreciation of a wide range of phenomena grouped together under the term “maternal effects”. “maternal effects” now refer to those long range influences on the biology of offspring (and even on the evolution of species) resulting from some particular aspect of maternal behaviour or physiology. By this expansive definition, the role of the egg in enabling (or initiating) fertilization can be described as a “maternal effect”, as can (and is so described) the role of the egg’s cytoplasm on the developing zygote. To illustrate the new importance “maternal effects” have acquired in evolutionary biology and ecology, let me read from two reports in the recent pages of *Nature* and *Science*.

In a review of William Eberhard’s recent book on sexual selection, T R Birkhead (1996) writes of changing perceptions of the role of females in evolutionary biology as follows:

> “Females have always had a bad deal in evolutionary speculation. When Darwin first proposed the concept of sexual selection, he imagined two processes: competition between males, and female choice. It was obvious that males fought for access

to females, but female choice was far from certain, and some people doubted whether females even have the mental ability to make such choices. It has taken more than a century of painstaking research to show that female choice is a subtle but important part of sexual selection.

. . . In 1970, biologists realized that there was more to sexual selection and that, even after copulation and insemination, competition between males could continue, through the process of sperm competition. Inside the passive female's reproductive tract, the sperm from different males grapple for paternity. Most behavioural ecologists were sexist, and it was more than a decade before the complementary idea of cryptic female choice emerged: i.e. that females might also be able to influence the paternity of their offspring. But even this ideal was too early for its time, and was generally thought of as barely credible. . . .

. . . A few years ago, a combination of events changed this perception. . . . By drawing our attention to the many different ways in which females may potentially control paternity (Eberhard's book), opens up a whole new field of research" (*Nature* **382** 8/29/96).

Elizabeth Pennisi (1996) has this to say in about new developments in ecology:

"(Experimental ecologists have long been) tripped up by a phenomenon that has often been observed but – until recently – rarely seriously considered: a so-called "maternal effect" which occurs when something about the mother's environment alters how her offspring look, act, and function. . . . Maternal effects . . . are proving to be much more than obstacles that occasionally confound experiments. . . . Maternal effects can enhance an offspring's chances of survival, skew sex rations, and drive fluctuations in population size. . . .

Researchers had been aware of maternal effects for decades, . . . but for the most part, these early workers viewed these effects as 'random noise that tended to obscure the genetic variation that we were interested in', says Mousseau. Thus animal breeders and evolutionary biologists would first grow several generations of the organism they wanted to study in controlled conditions so as to eliminate this 'noise' (*Science* **273**, 273, 6 September 96).

And in yet another reiteration of the theme, Mousseau and Fox write, two years later:

"Recently, the adaptive significance of maternal effects has been increasingly recognized. No longer are maternal effects relegated as simple 'troublesome sources of environmental resemblance' that confound our ability to estimate accurately the genetic basis of traits of interest. Rather, it has become evident that many maternal effects have been shaped by the action of natural selection to act as a mechanism for adaptive phenotypic response to environmental heterogeneity" (1998, 403)[6].

4. Discussion

Clearly, this is an exciting moment in biology. But here is the question: How did these shifts affecting the substantive research agenda come about? I certainly would not want to argue for a single factor explanation (e.g. Developmental Biology), but given the consonance between these developments and feminist goals, one is entitled to ask, did feminism itself play a role? And if so, What kind of role did feminism play? No one would argue for direct political action here – i.e. no committees formed to represent and advocate equal time for the egg, the cytoplasm, or the early (maternal) environment. However, there are two other kinds of arguments for a more or less direct effect of feminism that are made, and I want to address these:

(i) The shifts were introduced by the women scientists themselves. That is, the entrance of women in science, in large numbers, made it possible for a 'feminine' perception of the world to find its place in science. Indeed, some women in developmental biology have made precisely this argument – women as defenders of the egg. (As many women primatologists have also made.)

The Nüsslein-Volhard story is in fact instructive here, precisely because of her strong ambivalence about feminism, women and gender, and I think it can be used to illuminate the role that all of these have played in Developmental Biology. The basic argument I have made is that once the linear narrative – beginning with fertilization and ending with maturity – is disrupted, as it has now been, and the linear progression can be replaced by a circular one in which neither chicken nor egg can any longer be prioritized. And despite her extensive and multifaceted ambivalence, Nüsslein-Volhard was so placed in time and in both personal and cultural space that she was able to play a significant role in bringing this shift about.

Nüsslein-Volhard is known for her fierce ambition, her aggressivity, and her intolerance of those either unable or unwilling to survive on the "cutting edge" of what she regards as "good science", and because of this, some have argued – notwithstanding either her fondness for serving

[6]Citing Wade (1998), the authors go on to note that maternal effects might also have profound effects on evolutionary dynamics, promoting, e.g. runaway processes and rapid speciation (p. 406).

tea and for baking Christmas cookies or her focus on the developmental importance of "maternal effects" – that she has betrayed a "feminist vision of science", that she practices her science "just like a man." But in the final analysis, it needs to be recognized that neither of these notions have much meaning apart from their context. The particular meanings of gender, of feminism, and of science that are relevant to Nüsslein-Volhard are functions of the particular subject positions that have been available to her, and these are not only specific to her historical and cultural location, they are also multiple and contradictory. She identifies herself simultaneously as "scientist" and as "woman," where, as Hollway (1989) has emphasized, the meanings of each of these terms are themselves multiple and variable. The images evoked by "scientist" oscillate among images of an American style molecular biologist, a new breed of modern German biologist, and a more traditional German developmental biologist, while the images of "woman" oscillate between "American-style woman scientist", "path-breaking feminist", adversary of German feminism, caring "mother," independent "grandmother", "lonely" victim of German sexism. And the form of this oscillation – her frequent shifts in subject position – depend, as became abundantly clear in my interviews with her, on the particular context, the particular struggles (or power-relations), in which she finds herself engaged.

Perhaps the real moral of Nüsslein-Volhard's story is to be found in her very ambivalence. She has not needed to be an unequivocal supporter of either feminism or of women in order to make an intervention of immense value to women in science, just as she does not need to be an explicit proponent of a new discourse for the work she has done to play a pivotal role in dislodging the discourse of gene action. In turn, she has not needed to embrace feminist concerns in order to benefit from what such concerns might lead us to regard as victories. Gender matters to this story not because of her intent, but because of her situatedness, as a woman, in a field in which gender (now biologically, socially, and culturally) has mattered for a very long time – both for its practitioners and in the culture at large. Once again, gender matters for women in science not because of what they bring with their bodies, and often not even for what they may bring with their socialization, but for what the cultures of science bring to community perceptions of both women and gender – and in turn, because of what such perceptions bring to the communal values of particular scientific disciplines.

The days when one might have expected the needs and goals of women and feminists to naturally, as it were, cohere, or even that one might speak of the needs and goals of either "women" or "feminists" in a single breath, are long since gone. The great strength of feminist scholarship over the last decade has been the deepening of its understanding of what I might call the "situatedness" of gender. We have become exceedingly wary of sentences that begins with "Women are . . . ", realizing that just about the only way one can complete such a sentence is to say that women are people, situated by many social variables, and both adaptive to and resourceful in the face of the pressures and opportunities they encounter. For women working in Developmental Biology over the last couple of decades, some of those opportunities arose out of the social movement we call second-wave feminism, others out of different kinds of political, methodological, or conceptual/linguistic shifts that have taken place at the same time.

(ii) Another argument that is sometimes made is that we – feminist scholars writing about science – have helped to bring these shifts about. I wish I could believe that. Why not. Rather, I would suggest that these shifts were a consequence of the opportunities opened up by the social change that second wave feminism brought about. Scientists after all are human, and, in a sense, hunters. They are by necessity always on the lookout for a new angle, something that might give them an edge. The social change that feminism brought about provided new angles, new ways of seeing the world, even ordinary things, it opened up new cognitive spaces. And scientists jumped. You didn't have to be a woman to see or take advantage of this opportunity, and you didn't have to read feminist scholarship. All you had to do was to be a member of a different culture, all you had to do was watch the new strong women on the TV sitcoms of the 1980's. Indeed, I think that both of these arguments seriously misunderstand the nature of social change.

For all the difference that feminist scholars have made (and I include myself), however insightful our/their contributions have been, I want to argue that the real agent of change – if you like, the real heroine of the past three decades – has been the social movement itself. Indeed, feminist scholars are themselves – now and have been from the beginning – a product of that movement – especially, in the US. Of course, it goes both ways, but it is a noteworthy historical fact that, in this country at least, the emergence of feminist scholarship (and more, specifically, the subject of 'gender and science') was in fact preceded by a political and social movement. To be sure, the feminist movement began with the efforts of a few individuals and groups, but very quickly, it took on a life of its own, sweeping into its active center all the cultural machinery of a generation (indeed, that is precisely what made it a social movement). And feminist scholarship was only one of many of its by-products. The maelstrom of second-wave feminism gave rise to a men's movement, to a generation of caring fathers, to a profusion of new women detectives (both in novels and on television), to new forms of speech, to new legislation, to new social mores. In a word, it transformed the meaning of gender. One of the most dramatic by-products of this transformation, especially in the context of gender and science, was the opening of science, engineering, and medicine to women, and the dramatic influx of at least white women into these arenas. But another by-product can be found in the kinds of substantive shifts I have been describing.

Have the women themselves changed the doing of science? Have they by their own example brought a new legitimization of traditionally feminine values into the practice of science? Thus posed, my guess would be: probably not. With a few possible exceptions, I do not believe that women scientists have either sought to or succeeded in introducing stereotypic feminine values into the lab – indeed, logic itself seems to me to argue against such a possibility. As the most recent group to be integrated, women scientists are under particular pressure to shed whatever traditional values they may have absorbed qua women – if for no other reason, then merely to prove their legitimacy as scientists. But if we were to rephrase the question and ask, has their presence helped to restore equity in the symbolic realm in which gender has operated for so many eons?, I would answer with an unequivocal *yes*. Especially, I would argue that the commonplace presence of women in positions of leadership and authority in science has helped erode the meaning of traditional gender labels in the very domain in which they worked, and for everyone working in that domain

As I have already said, this is a tremendously exciting moment in biology. As it is in society. And at least in part, we have the women's movement to thank. "Second wave" feminism has been one of the most powerful social movements of modern times (perhaps especially in the US). And the changes it helped bring about have been massive. Even so, I wish I could claim more.

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